

# SCIENCE

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## ARCHÆOLOGY 1880-1892.

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### Egypt—Mr. W. M. Flinders-Petrie.

IN the last few years practical archæology has made rapid strides; and — thanks to the increasing number of well-qualified scholars who to-day fill its ranks, as well as to the consequent precision of its improved methods — it not only may now be numbered among the natural sciences, but a general knowledge of its results has become a necessary part of the intellectual outfit of every man who would lay a claim to keeping abreast of the times.

Scarcely a year passes without bringing some new and startling discovery that sheds a vivid flash of light upon some hitherto obscure corner of the remote past. Ancient Egypt, Mesopotamia, Syria, the Mediterranean islands, Asia Minor, the palæolithic and neolithic human strata of pre-historic Europe — all of which, fifty years ago, appeared to the student as isolated reefs, here and there cropping up out of the dark surrounding abyss of time, apparently unconnected with one another, and having but little if any bearing upon our own civilization — are now assuming a new interest. Each is gradually taking its proper place in the great scheme of history; and, in so doing, many of the human fossils, whose dismembered forms, viewed singly, seemed abnormal and fantastic, are resuming their natural aspect. The so-called giants of the past, examined in the light of modern criticism, are shrinking to a normal size; the mythical races have lost their fabulous character; the wild savage has given place in scientific literature to the non-civilized man; the barbarian to the man of low culture; and all have become well-connected links in the long chain of classified human development.

Civilization, in the earliest times as at the present day, has been found to be a sliding scale; and we now know that, in the fourth millennium B.C., as in the nineteenth century A.D., there might be found upon the surface of the globe every grade of culture from the highest, which found expression in the inscribed monuments of Egypt and Chaldea, to that whose remains might be sought among the shell heaps of the far-north.

The pre-Homeric Greeks and the other inhabitants of pre-historic Europe and northern Africa are no longer ignored as little better than savages. As they take their proper place in the history of humanity, Egypt ceases to be spoken of as a "Miracle in Stone;" and — if it still stands pre-eminent among the nations of the ancient world — for the modern scholar it now only represents the crest of the irresistible onward wave that, at a given time, carried human thought to the point where the advancing genius of the Mediterranean races took it up and idealized it, before receding in its turn and leaving its future progress to the peoples of the north.

The recent researches of anthropologists, interpreting the facts furnished by the pre-historic burials of Europe, have everywhere strengthened the view which connects its former inhabitants with the populations of the historical period. Types that once seemed widely different are found to be connected by intermediate ones; and a continuous line, reaching back through ages, has in many instances been formed by the recovery of missing or of hitherto misinterpreted evidence.

As the question now stands, those who would still cling to the hypothesis of Indo-Germanic migrations pouring down into Europe from the plateau of central Asia, must be prepared to face an immense body of facts which they will find equally difficult to get over or to disregard. The theory of the Asiatic "cradle of

the Aryans" was originally accepted upon the authority of such eminent philologists as Pott, Lassen, and others; and, despite the early protests of a few scholars (see Dr. D. G. Brinton in *Science*, June 24, 1892), its hypothetical nature was lost sight of and it soon found its way as an undisputed fact into our hand-books and primers. Even to-day — although the word "Aryan," divested of its former meaning, has mainly become a term of philological classification for those who are familiar with the subject, and notwithstanding the arguments to which the use or the misuse of the word has publicly given rise — this curious myth of the nineteenth century still holds the position of an orthodox belief, and is still taught as an article of faith in our schools!

In the face of such facts, and as the results of archæology have a recognized and direct bearing upon many branches of scholarship, it is of serious moment that these results should be published, not only as speedily as possible, but in such a form as to make them available to all.

At present this is rarely the case. As a rule, the labors of archæologists working in different fields are published either in large works, the technical details of which are well calculated to frighten all but specialists, or in some one of the many scientific journals edited in all parts of the world. The number of these, however, is increasing yearly; and this fact considerably diminishes the chance of each to reach all those interested, again excepting the specialist, who of course makes it a point to see all that is written in reference to his own line of research.

Owing to this, much that is of vital consequence to the intellectual world at large is passed by or often remains long unnoticed by an important portion of the community.

It is therefore greatly to be desired that archæologists, who wish their labors to be of as much use as possible, should follow the example given them by Mr. W. M. Flinders-Petrie. This indefatigable scholar — with whom to drop the spade is but to take up the pen — has found the time, in the midst of his more serious scientific work, to publish a *résumé* of his recent explorations, under the title of "Ten Years' Digging in Egypt."

This little book will not only be useful to students, but must prove delightful reading to all who are interested in the progress of scientific discovery. Moreover, although condensed, as it necessarily must be in order to carry out the intention of the author, it is extremely suggestive, and opens to the mind possibilities most stimulating to every thoughtful student of the past.

In ten chapters the explorer lays before his readers the well-sifted residuum of new facts acquired to science through his excavations at Gizeh, Tanis, Naukratis, Daphnæ, Nebesheh, Hawara, Illahun, Kahun, Gurob, and Medûm, dwelling only upon such "finds" as distinctly bear upon the life, the history, the arts, and industries of man in the valley of the Nile, at the various periods represented by those sites.

It would be difficult to discriminate with regard to the relative value of these discoveries, each of which has elucidated some obscure point of science. Much light has been thrown upon the geology and the formation of the Nile valley as well as upon the climatic and topographical changes that have taken place since pre-historic times.

Valuable information has also been gained with regard to the tools and the devices employed by the Egyptians in the carrying-out of their artistic and architectural works, and upon the technical details of their earliest monuments.

The careful examination of the pyramid of Medûm — the tomb of King Seneferu (third dynasty), has established the evolution of the pyramid from the mastaba; and the discovery of its temple has placed before us, intact, the oldest-dated edifice in the world.

Moreover, the close observation of the well-executed hieroglyphs found upon the walls of the tombs of this ancient nekropolis has given many useful hints as to early Egyptian customs. For instance, the fact that the numerals turn out to be formed of lengths of rope, has suggested to Mr. Petrie the possibility of an original reckoning by means of ropes — a custom found in other parts of the world.

At this remote period stone and copper tools were used; only one bronze implement having been found in the course of the excavations. This exceptional piece Mr. Petrie regards as a trade importation; if, indeed, as is more likely, its presence is not to be accounted for by displacement from a higher level—it would be difficult to imagine whence true bronze could be imported in the fourth millennium B. C.

Under the reign of Seneferu, the Libyan already appears as the civilized ally of Egypt; as is testified by the familiar hieroglyph of the Libyan soldier armed with his bow and arrow.

Traces of two distinct races — represented by distinct funeral customs — were also found among the graves near this pyramid. From these were obtained fifteen skeletons, five of which present cases of mutilation or deformity. The series is now at the College of Surgeons, where it will be studied and reported upon.

The other chapters of Mr. Petrie's book, in which he treats of his excavations in the Delta and in the Fayûm, are no less interesting. They furnish the reader with the principal data upon which the learned explorer bases his conclusions as to the relation of Egypt to the rest of the ancient world; and they naturally lead up to the eleventh chapter, in which these conclusions are expressed and vigorously set forth.

It was already fully established, by inscriptions found upon the Egyptian monuments, that the Mediterranean races were in contact with Egypt under the New Empire. Mr. Petrie has not only added to the evidence: he has recovered traces of foreign colonists residing in the Nile Valley, and has brought forward proof that close intercourse existed at least as early as the fifteenth century B.C., with a strong presumption in favor of the view that such intercourse must be carried back to a much earlier period.

From the facts which have come to light in Egypt and elsewhere, he argues that the civilization of the Bronze Age arose in Europe; that the use of bronze was introduced into Egypt by northern, not eastern, contact; and that it is from the mines of Hungary, Saxony, and Bohemia that the tool-makers of Pharaonic times derived the tin which they used as alloy in their industry.

According to Mr. Petrie, Europe evolved its own culture as independently as Egypt itself, and in its relations with other civilizations it gave the East as much as it received. It is indeed to Europe that we must look for the origin of the bronze age; and there is little doubt that, if the place occupied by the primitive culture of Europe has so long been overlooked and ignored, it is principally because it did not evolve a graphic system, and therefore remains silent when the monuments left by the Oriental civilizations are bursting into words. Yet, "if silent, it is not dumb." The Mykenæ culture has left traces that lead us back at least to the sixteenth century B.C. (its decadence had already begun by 1100 B.C.). It was not confined to Greece; it was far-reaching, and represents the highest outcome of the bronze age. Its influence was felt from the Mediterranean to the Baltic, and it came into contact with Asia as well as with Africa.

"This bronze age," says Mr. Petrie, "is the source of the objects we now use. Thence these types were carried into Egypt a couple of centuries later by the Greeks. When we descend further, we see this independent culture of Europe prominent. The Saxons and the Northmen did not borrow their weapons, their laws, or their thoughts from Greece or Italy. The Celts swamped the south of Europe at their pleasure; and against the fullest development of Greek military science they were yet able to penetrate far south and plunder Delphi. They were powerful enough to raid Italy right across the Etrurian territory. When we look further east, we see the Dacians with weapons and ornaments and dresses which belong to their own civilization, and were not borrowed from Greece. In short, Greece and Italy did not civilize Europe, they only headed the civilization for a brief period."

Such are the conclusions reached by Mr. Petrie after ten years of patient investigation and of a most serious study of the Egyptian written and unwritten record. In matters of detail Mr. Petrie's interpretation of certain facts may be open to discussion; some time will probably elapse before the exact dates suggested by him for some of his "finds" are definitely adjusted to the entire satisfaction of experts in special branches of culture; but, in their bearing upon the history of civilization, his views may broadly be said to fit in with the general results of archaeologists elsewhere.

Before the time of Dr. Schliemann's explorations, the Mykenæ culture had yielded so little, and its extent and influence were so ill understood, that the strong individuality displayed in its art was, if not unnoticed, at least unappreciated. With a better understanding of its remains, this art, wherever met with, reveals an originality of methods and of technic which cannot be denied, and its archaism has become evident.

It not only produced tombs, the grandeur of which outshone the dwellings of the living; and massive stone palaces, the walls of which were decorated with painted frescoes, — the work of a school whose artists borrowed neither their subjects nor their mode of treatment from others, — but metallurgy had reached a high degree of development; bronze, silver, and gold were wrought with the greatest skill; the engraver displayed a true artistic sense in the cutting of gems; and the manufacture of fine painted vases did credit to the originality of the Mediterranean potters. Altogether we are here in the presence of men who, in the course of their evolution, may have received suggestions from the outside world, but who never were servile imitators; and who, in their relations with other civilizations, "gave as much as they received."

#### Cyprus.—Dr. Max Ohnefalsch-Richter.

In Cyprus, the well-known explorer, Dr. Ohnefalsch-Richter, has detected — below other ancient human deposits found upon the island — the presence of a Phrygo-Thracian civilization which belongs to the copper-bronze age.

This he regards as having certainly preceded all contact with Mesopotamia, Egypt, or Phœnicia; and as presenting many affinities with the kindred culture of which the late Dr. Schliemann found the traces in the lower strata reached in his excavations at Hissarlik — the site of ancient Troy. Dr. Richter's view is accepted by other authorities, notably by Dr. Dümmler, who goes even further, and regards both cultures as identical.

It is only later, under Sargon I. of Akkad, and Naram-Sin, his successor, that Mesopotamian influence can be detected upon the archaeological record of Cyprus; and it is later still that direct contact with Egypt and with the Hittite Empire makes itself felt.

According to the facts brought to light by Dr. Richter, Greek influence antedates the oldest traces of Phœnician intercourse with the island; and the Greco-Cyprian syllabic character occurs in process of formation in the copper-bronze stratum, i.e., an earlier date than the first evidences of the use of the Phœnician character, which are contemporaneous with the introduction of iron. This seems to coincide with the downfall of the Hittite Empire, about 1300 B.C.

Dr. Richter concludes from these and other facts that the primitive civilization of Cyprus was evolved independently of the East, and that it must be regarded as running in parallel lines with those of Egypt and of Mesopotamia. This culture may be traced through Asia Minor, the Greek Archipelago, Greece, and Europe at least as far as Königsberg. Its oldest vestiges in Cyprus he dates as far back as the third or fourth millennium B.C. Beyond, there are evidences of a preceding stone age. Of these earliest remains Dr. Richter makes two divisions. The oldest stratum produces no statuary, no inscriptions; but it contains hand-made pottery, and copper or weak bronze tools of small dimensions, in the smelting of which a weak alloy of zinc was used. In the second, or later, stratum of this archaic period, lance-heads appear, and with these are found vases of the Mykenæ type.

According to the facts which he has collected, the Greek influence felt here is that of the Peloponnesian tribes, the Arcadians,

Achæans, and the Laconians, whom he recognizes among the nations mentioned in the Egyptian texts (Akuaivasa, Sakalusa, etc.). It is to these, it would seem, that was due the invention of the sword, which first appears in Cyprus in the form of a much enlarged Cyprian dagger, and is found associated with Mykenæ pottery. It is also at this time that defensive metal armor occurs. Many of the statues of gods, priests, and others exhumed by Dr. Richter are represented wearing the round helmet with a ball-crest, which is depicted upon the Egyptian monuments as especially belonging to the Mediterranean peoples; and their general aspect also agrees with that attributed to these races by the Egyptian artists.

It will be remembered that these round helmets are mentioned, along with coats of mail, in Homer; and that, in the great epic, Agamemnon is represented as having obtained his equipment from Cyprus. The Cyprians continued famous as metallurgists, and, even after iron was introduced, their swords remained celebrated—at least we may judge so from the fact that Alexander the Great is stated to have carried a Cyprian sword.

Dr. Richter is now engaged upon the publication of a large illustrated work which will give a full account of his discoveries, with his interpretation of the facts that he has had the good fortune to bring to light in the twelve years of his Cyprian explorations. It is sincerely to be desired that he may be induced to follow Mr. Petrie's example; and that, along with the exhaustive volume more particularly intended for specialists, he will also publish a *résumé* embodying his principal results.

#### CLIFF- AND CAVE-DWELLERS OF CENTRAL ARIZONA.

BY J. W. TOURNEY, TUCSON, ARIZONA.

THERE is no part of the United States that is of more interest to the archæologist than our great south-west. This region, which includes all of Arizona, has but little rain, and during the long summer lies scorching under an almost tropical sun. It is true that in the mountains of the Territory the heat is greatly lessened, but from our geographical position we are subject to prolonged droughts for weeks and months at a time.

It is a question whether our present meteorological conditions are the same as when the cave- and cliff-dwellers cultivated fields upon our now barren mesas. Throughout this vast territory are hundreds of ruined casas and vacant caves, whose silent walls and rough-hewn stones are the only history we have of this early people. Whither they have gone and who they were is shrouded in the grave; neither the fierce Apache nor the more gentle tribes of the south know who were the architects of these interesting and wonderful structures. The Zuni and Moqui have been questioned, but without result. They all shake their heads. Even the oldest of the Indians, with traditions extending back many generations, know nothing of the builders of these old stone and adobe walls which have withstood the elements for so many years. At some future time these silent walls may speak out through the mind of man and give to the world glimpses into the history of a people who are now lost in antiquity. Peculiar marks on rocks and pottery all mean something, and the world is waiting for someone to unlock these closets and let a ray of light upon their mysterious symbols.

In the valley of the Verde River, not far from the now abandoned fort, and on the surrounding cliffs and hills are many old ruins. Many of these are as interesting and as little-known as any to be found in the south-west. Far from railroad and in a wild and rugged region, it is not strange that they have been studied so little. The banks of the river and many of its tributaries are lined with ruins. The prominent cliffs are crowned with walls of great flat stones, many of them as erected ages ago.

The clear water of Beaver Creek reaches the Verde a few rods above the old fort. A ride of several miles along this creek brings one to high cliffs extending a hundred or more feet above the waters of the creek. In the perpendicular walls of one of these cliffs is a well-preserved ruin known as Montezuma's castle. It is midway between the rim of the cliff and the bed of the stream, and is neither house nor cave, but a combination of the two. Not accessible from the summit of the cliff, it can only be reached

from below, and even here not without the use of a ladder, which, if short, must be pulled up from one ledge to another in making the ascent. The entire front is of artificial walls built of large, flat pieces of limestone, with openings here and there for doors and windows. The rooms are small, only about five feet to ceiling. Generally a small opening two or three feet in diameter joins one room with another, and a similar orifice in the ceiling

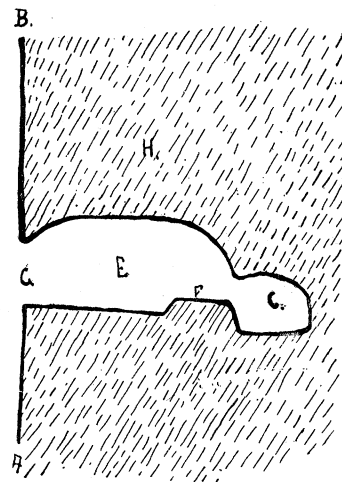


Diagram showing vertical section of cave at right-angles to cliff. The line *AB* represents face of cliff; *C*, a small cave at rear; *E*, main cave; *F*, rock bench; *G*, entrance; *H*, rock of cliff.

gives access to the room above. The ceilings are so low that steps are not necessary to pass from the first floor consecutively through the several stories of the structure. The openings in the ceilings never fall directly under each other. If the orifice is in the north-west corner in the first ceiling, it will be in the south-east corner in the next ceiling above, and so alternating back and forth to the top. This of course eliminates the possibility of falling more than five or six feet, or the height of one story. The

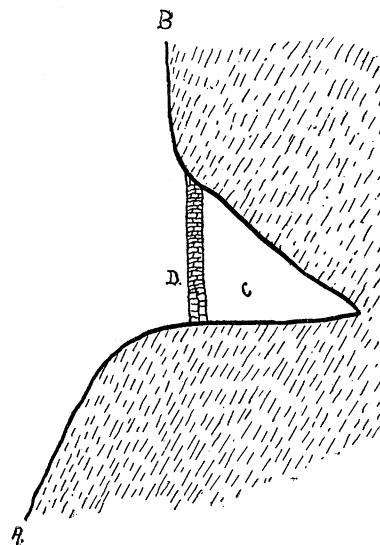


Diagram showing vertical section through large horizontal fissure; *AB*, line representing face of cliff; *C*, enclosed room; *D*, wall of masonry.

Type of pre-historic dwelling along the Grand Cañon of the Colorado River.

floors are mostly of flat stones supported on timber cut from the surrounding mountains.

Many of the timbers are perfectly sound, while others, where the rains have beaten against them, have decayed away and the floors fallen in. A few of the rooms facing the creek have cross-walls built of limestone, without mortar or cement of any kind. They all show considerable skill in their construction. The rooms at the rear are dark, dungeon-like caves hollowed from the solid

rock, and are now the abode of thousands of bats, which fly about in great numbers when disturbed by the sight-seeker.

A few miles above Montezuma's castle, on the opposite bank of the creek, a conspicuous cone-like mountain rises a few hundred feet above the surrounding country. On making the ascent, the summit was found to be a narrow rim enclosing a crater some three hundred feet in diameter and with nearly perpendicular walls. Standing on the rim, one can look down a hundred feet upon the dark-blue water of a small lake in the bosom of the mountain. The lake, a hundred yards in diameter and of unknown depth, is known as Montezuma's well. In the steep sides of the crater are a number of caves, which at one time were the abode of man. A few are natural, but the greater number are the result of human effort.

The rim is crowned with the fallen walls of an ancient ruin more than a hundred feet long. Far down the mountain-side, below the level of the water in the crater, the outlet of the well flows from between an opening in the rocks. This stream is large and constant, and at present is used to irrigate a ranch in the valley below. Ages ago the builders of caves and castles utilized this same stream to irrigate portions of the neighboring rich valley.

A short distance down the valley a stone and cement ditch of pre-historic make can be easily traced for many rods. Ranchmen in building ditches frequently follow the courses of ancient ones. In July, last year, in constructing an irrigating ditch near old

from one and one-half to two and one-half feet below the floor of the main room.

It is probable that the small caves were used for the storage of grains and other material. No light finds its way into the small lateral and rear caves but the little that comes in through the small openings leading to the central room. In two or three instances I found two large caves joined by a small passage-way uniting the lateral caves. Occasionally, hollowed from the wall, at one or both sides of the main entrance, some two feet above the floor, were small pocket-like cavities about twelve inches in diameter and nearly spherical in outline. The openings to them were four or five inches across, so large that one could easily reach with the hand any object that might be placed therein. Not only the floors of the caves, but in many instances the entire face of the cliffs, were covered with broken pottery, some of it of much better quality than that made by the Indians of Arizona to-day.

So far as I have had opportunity of examining, the caves of this region are much different from those in the cliffs along the Colorado River and elsewhere in the territory. Here it is evident they have been hollowed out by human effort. In other localities natural caves and large horizontal fissures in the cliffs were the homes of this early people.

#### PALÆOLITHIC MAN IN NORTH AMERICA.

BY DR. C. C. ABBOTT.

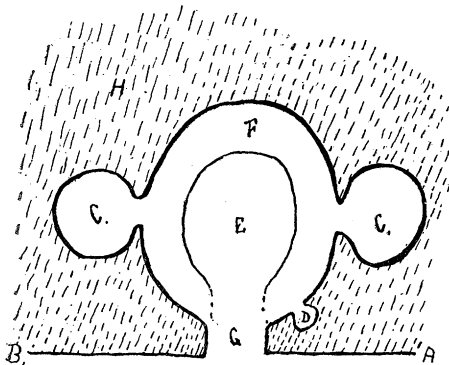


Diagram showing floor-plan of cave. The line *AB* represents face of cliff; *C*, lateral caves; *E*, main cave; *F*, rock bench surrounding main cave; *G*, entrance; *D*, small cavity hollowed in rock near entrance; *H*, rock of cliff.

Fort Verde, on the west side of the river, the workmen found evidences of an ancient ditch buried some twelve feet below the surface. Many of the old ditches have been found upon mesas where under present conditions it would be impossible to get water to fill them. Frequently they lead from what are now dry washes which only carry water a few days at a time and then only after heavy rains. This seems to indicate that there was a time when the now dry washes carried water much more constantly than at present.

More numerous than the casa and cliff ruins are the many caves excavated from the sand and limestone cliffs along the east bank of Verde River for some miles below the old fort. At a distance the openings into the caves look like black spots on the white cliffs. They are arranged in long rows, tier above tier, and are nearly alike in structure. All are more or less inaccessible from the valley below. The entrance is an irregular arched opening about four feet high and from half to two-thirds as wide. The cave proper is about twelve feet in diameter and from four to six feet to ceiling. The room is more or less circular in outline. A rock bench from twelve to eighteen inches high occupies the cave on all sides except at the entrance. This bench is about three feet wide, and gently slopes toward the centre of the room. Charred embers, meates, grinders, broken pottery, and fragments of reed mats were scattered about or were under the heaps of *débris* which covered the floor. Opening into the main cave at either side and also frequently at the rear were smaller ones, which were three to five feet in diameter and about the same to ceiling. In nearly all the caves visited the floors of the smaller ones were

THE ghost of palæolithic man has arisen to plague the geologists at Washington; and those that look upon them as little gods are all shouting "Me, too." As the cause of all this mischief, it is fitting that I should speak in my own defence. The scientific men of Washington claim a monopoly of knowledge and so occupy a peculiar position, self-assumed, of course. That which is offered the world independently of them, must be stamped by their approval or condemned, and it is never the former. This condition of affairs really handicaps them at the outset, and not one can enter the field unbiassed. Indeed, they go out instructed to bring in such and such results, and none other. This is pre-eminently so in the question of the antiquity of man in North America. The recent appearance of Wright's book, "Man and the Glacial Period," has set their pens and tongues wagging, but palæolithic man is not to be downed even by such an array of notables marshalled to defeat him. Salisbury's cunning argumentation, McGee's shaggy front, Holmes's imperious "begone!" and Brinton's persuasive smile do not make him afraid. He returned to earth in his own good time and came to stay!

Of the alleged evidences brought forward by others I have nothing to say, but something to record concerning my own investigations, that may have a bearing on the question. We must admit that, at some given day in the past, man appeared on this continent; but just when, no one has ventured to assert. Certainly in no one communication to scientific or popular literature have I done more than claim the discovery of evidence of his comparatively primitive condition when he did arrive; and now after twenty years of careful, unremitted study of the valley of the Delaware River, I see no reason to change my opinion, but a great deal to substantiate it.

Were the evidences of man's occupancy of this region one associated and confused mass, an attempt made to dissociate its components into rude and more elaborate forms and to say of the former, *this is old*, and of the latter, *not so ancient*, then the scientific world might well be up in arms and cry down the apparent absurdity — but this is not the case.

Of course, if we claim, as, for instance, Mr. Holmes practically does, in spite of denial, that every so-called palæolithic implement is a "reject," whether the man who threw it aside lived in Europe or America, the whole subject falls to the ground; but accepting palæolithic man as a one-time feature of other continents, and believing no geological reasons have been brought forward why he might not have lived in North America also, it is justifiable to consider the archæological significance of such objects as the late Wyman said were not distinguishable from European forms, except by the material of which they were made.

Now, as a matter of fact, a considerable number of just such forms have been found in the gravel deposits at Trenton, N.J., and at a significant depth; but, says the geologist, what of the age of this deposit? The whole question hinges on this. Professor Salisbury asserts that since the deposit was originally laid down, it has been reassorted. Grant this, and what then? If the reassortment took place in "Indian" times, how does it happen that only this one form and simple flakes are found entombed? Holmes here steps in and says "easy enough," the Indian went to the river-shore, chipped pebbles, and retired to the back country, leaving his "rejects." But are we to suppose the Indian never went to the water's edge for any other purpose? Did he not take his finished implements to the river to fish and hunt? Did he not cross the river by a raft, canoe, or by swimming? Did he necessarily always live back from the stream? Common sense points out that he must have had the whole range of his goods and chattels continually at and on the water, and are we to suppose that never a knife, arrow-point, bead, or pot was lost? It is too absurd to consider; and this reassortment of the gravel-beds must have buried a great deal more than "rejects." Again, it has been asserted that the assumed palæolithic implements are only in "talus." Carvill Lewis, according to Brinton, says what I held to be undisturbed layers, were really an "ancient talus." Possibly, but how ancient? In at least a dozen instances this asserted "old talus" was caused by floods having a transporting power equal to piling up layers alternately of sand and gravel, and then, as if to anticipate the present tempest in a tea-pot, placed a boulder, weighing tons, over it all, for fear that the poor palæolith might run away. Now, when grooved axes and polished celts are found under like conditions, I am willing to leave the field as fast as my short legs will permit, and not before.

Professor Salisbury has asserted that there is need of expert testimony to determine the precise age of the implement-bearing gravels, and Dr. Brinton insists that no opinion as to the geological age of a gravel can be received from any but an expert geologist. Grant it; but the trouble is these "expert geologists" are *rare aves* that were never yet known to agree among themselves, and it becomes a mere matter of personal opinion after all. I lay claim to a smattering of gravel-ology. I have lived on pebbles so long that I have become flinty-hearted so far as criticism is concerned, and when I find gravel stratified and unstratified, I know and assert the difference; and when a palæolithic implement is found in gravel beneath layers of sand and pebbles, beneath huge boulders (not merely at a lower horizon, but directly beneath them), I do not, and no reasonable person should want another to tell him that the two were laid down together, or the big boulder was dropped upon the implement, which anticipated its coming. Up pops some "authority" and declaims the possibility that the ground was washed from beneath the big stone and the implement slipped in. Well, we can go on supposing till the crack o'doom, but as to proof, that is another matter. These geological jugglers will prove yet that the Indians bought the Delaware Valley from William Penn.

Certainly too much value is put on this matter of expert testimony. Then, again, in spite of all that has been written and said, the assertion is made that palæolithic implements are found only at the present river-shore. Of course we find them there now, because the gravel is exposed, but not there alone. A full mile back from the river they have been found in digging cellars, sinking wells, and in the cut of the Pennsylvania Railroad, east of Trenton, N.J. All this area may have been "reassorted," but in such delicate fashion that the strata are not broken, and it suggests that the manner of it was like turning over a book from one cover to the other.

Again, it has been objected that no animal remains have been found; but Cook found a mastodon, and I have, more significant yet, a valve of a *Unio*; and what of human remains, long since reported? There are, too, at the Peabody Museum, three human crania, two of which were taken from the gravel and one found in the bed of a creek, and these three, identical in character, stand alone in a collection of nearly three thousand Indian crania.

It is the weak point of Wright's book that he did not prepare the archæological portion at the Peabody Museum, with my col-

lection under his eyes. If he had, the critics would not have had a leg to stand upon.

The implements, too, speak for themselves. If "rejects" as Holmes dogmatically asserts, why is it that they were carried to the high ground, and are found to-day, solitary and alone, silent witnesses of that long ago, when it was the principal weapon of the early man who used them? And if "rejects," made at the water's edge, where are the chips resulting from their fashioning? They are not scattered broadside over the river-shore as are the implements; but we do find in spots where "rejects" were made in numbers, and know the fact because of the accumulated chips. It is easy to conceive a theory and bend the facts to it; very, very easy; but the trick is found out, sooner or later.

"But they show no sign of use" pipes some impatient kicker. Prove it; and does the spear or arrow-point show signs of use? Of over a thousand chipped jasper scrapers in the Abbott collection at the Peabody Museum, not a half-dozen show sign of use, and the same may be said of drills.

These rude implements are made of argillite, and the use of this material was continued down to the time of European contact, being less and less used after the discovery of jasper. The magnificent results of Mr. Ernst Volk's explorations, under the direction of Putnam, in the valley of the Delaware, clearly prove this, and so substantiate what I have claimed for all these years; and is it not significant that some of the most finished specimens of palæolithic implements have been found *in situ*? By what authority do the critics say they are too rude to be effective? Is any person living so in touch with primitive man to-day as to assert what he could and could not have used? It is well to bear in mind that many an undoubted Indian implement, just as rude, was used by these later people. Look at the rude spades and slightly chipped but girdled pebbles that were used as club-heads.

Of course in the days of palæolithic implement-making there would be "rejects," and the critic must not attempt to prove too much, because such are found, even in undisturbed gravel. Many a pebble, too, has been chipped until suggestive of an implement, by the detaching of flakes to be used as knives, as Mercer pointed out at the Rochester meeting of the A. A. A. S., and a splinter of stone was not too elaborate an implement for supposed palæolithic man to have used.

And now, in conclusion, let us remember that the native American — the Indian — is a type distinct from all other peoples; let us not forget that their languages are all a purely home product, and that these facts show undeniably a necessarily long occupancy of this continent, shut out for centuries from all the world. If he, as a *fully equipped Indian*, came from another region beyond the seas, his similarity to the people of that region could be traced. As it is, he came, so far as our knowledge now extends, when man over the whole world was not racially developed as now, and so, when in a comparatively primitive condition; such a condition as is suggested by the simplest of implements, whether for the chase or domestic uses. Here, in North America, this early man became a potter, invented the bow, and gradually reached that status of culture, differing in degree in different parts of the country, in which he was found by European explorers.

As a student of archæology, I submit that this occupancy of the continent commenced when there was a changing condition of the river valleys in progress; but whether that change was subsequent to the glacial epoch or during it, deponent saith not. That it was during a time when rock-transporting floods were common, I do claim. That it was when ruder than ordinary Indian implements were the common tools of the people, I do claim, for how else could only such rude forms be associated as they have been shown to be with gravels that show no evidence of disturbance except such as forces not now in operation, effected? It is true, palæolithic and Indian objects are now associated, but they are also separate and apart. What I contend for is the sequence of events of the original use of a rude weapon or tool, the one implement of that day that was manufactured, and, as time rolled on, the production of more elaborate forms, and all that pertains, the world over, to the accepted neolithic stage of human advancement.



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## THE SPELLING AND PRONUNCIATION OF CHEMICAL TERMS.

BY THOMAS H. NORTON, PROFESSOR OF CHEMISTRY, UNIVERSITY OF CINCINNATI.

THE necessity of establishing standards with reference to the nomenclatures of the different provinces of science has been felt for several years past, with more or less force, according to the branch concerned. In geography our own government has taken a most desirable initiative by issuing authorized lists of geographical names, the spellings of which have been the result of careful study and adherence to a few fixed general rules. Much has been done of late towards the establishment of a uniform nomenclature in geology, while the botanists assembled in an international congress this fall to grapple with their phase of the problem. In medicine the necessity of standards for uniformity in pronunciation is felt most keenly, but no decisive steps have been taken. It is by no means uncommon for students in a medical or pharmaceutical college to hear widely divergent pronunciations on the part of the corps of instructors.

The existence of these diversities, not only in medicine but also throughout the whole range of the sciences, is due chiefly to two causes. The first is the radical change which has taken place in the pronunciation of the classical tongues during the past quarter of a century, and which has naturally exerted a powerful influence on the pronunciation of naturalized Greek and Latin terms as well as of most derivatives from these languages. The second cause is to be found in the effects of Continental—i.e., French and German—usage on the constantly-growing contingent of American scientific and professional men who have studied in European universities. Involuntarily they often retain the Continental pronunciation of the vowels—especially *i*, in a less degree *a* and *e*, and still less *o* and *u*—in the use of words of identical or similar spelling. When this happens in the case of instructors, their usage is of course widely imitated.

Among our chemists, the need of adopting standards has been felt chiefly in the following directions.

1. The rapid extension of organic chemistry has led to the discovery of a notable array of new classes of compounds, whose existence was totally unforeseen and for whose naming, naturally, no provision was made, when about thirty years ago our otherwise admirable system of nomenclature was introduced by Hofmann and his contemporaries. This problem is, of course, one essentially international in its nature, and is now fortunately in a fair way to be solved. At the Chemical Congress, held in connection with the Paris exposition of 1889, an able committee was appointed to carefully formulate the questions needing decision, and make suggestions as to their treatment. As the complement of their work a congress of representative chemists was held during the past summer at Geneva, that favorite meeting-place of international conferences, and the great majority of the questions were settled in a series of sixty-two rules adopted with practical unanimity. Time limitations prevented the completion of the

work, which is postponed to an adjourned session. It is impossible here to go into detail upon the important results of this congress. Suffice it to say that it has, with reasonable simplicity and deference to existent usage, provided a nomenclature which will meet the needs of chemists for probably 20 or 30 years. The chemist's language is not unlike that of the Turk, in which growth and change occur so rapidly that each new generation requires a totally revised and modernized edition of standard works in order to render them fairly intelligible to the reading public.

2. A settlement of the claims of priority in the case of the names of two elements, Columbium (or Niobium) and Glucinum (or Beryllium), seemed eminently desirable.

3. Equally important seemed to be the adhesion to several decisions on minor questions in terminology, such as that of the alcohols, the use of *-ic*, etc., already adopted by the London Chemical Society.

4. A subject of prime importance was the adoption of some fixed spelling and pronunciation for certain terminations, notably *-in* and *-ine*, *-id* and *-ide*, which would effectually banish the present lack of uniformity and adherence to the ordinary laws governing word-building and pronunciation in our language.

5. It seemed also proper to ascertain how far the chemist can go in adopting the simpler forms of spelling advocated by the Philological Societies of Great Britain and America, availing himself of the resultant economy and keeping in touch with the evident steady progress of phonetic reform in the English language.

For the purpose of obtaining a consensus of opinion and ultimate decision on the part of American chemists with reference to the four latter topics, the Chemical Section of the American Association for the Advancement of Science appointed in 1887 a special committee, which later, on account of the importance of the subject, was made one of the standing committees of the Association. Since that time the members of the committee have been in active correspondence with the entire body of American chemists and leading philologists, by means of annual circulars and individual communications, while at the successive meetings of the association the subject has been a regular topic for discussion. The final report, embodying the results of these few years of work, and approved unanimously by the Chemical Section of the Association, has recently appeared in print and been widely disseminated.

The importance of obtaining uniform usage in the application of these rules has been so fully recognized that the Bureau of Education at Washington is issuing an edition in the form of a small wall-chart, to be distributed to high-schools and colleges, which can thus keep the authority constantly in view in lecture-room and laboratory.

It might be added that the chemical nomenclature of one of the largest dictionaries in our language, now in course of preparation, is based upon this simple code, which has likewise been adopted by the influential *Journal of Analytical and Applied Chemistry*, and also used by Dr. T. Sterry Hunt in his latest work upon "Systematic Mineralogy," and in Professor R. A. Witthaus's recent "Manual of Chemistry." Since the appearance in print of this synopsis of rules, the writer and other members of the committee have received frequent inquiries with regard to the exact reasons underlying one or another of the individual changes recommended. These inquiries have come from those who have lacked the opportunity to keep *au courant* with the progress of the discussion and the final decisions.<sup>1</sup> It may, therefore, meet a

<sup>1</sup> This lack of general information on the subject and familiarity with the careful, cautious and conservative spirit in which all suggestions of change have been made, is well illustrated in a recent communication to this journal (p. 247). In this the writer, having encountered *sulfate* demands why *phenolphthalein* does not also undergo change, and then seeks to "picture our laboring scientists, with the new-system dictionary before them, ever fearful of beginning one word with an *F* after the new, and the next with a *Ph* after the old system." He is evidently unconscious of the one fact that the simplified spelling of *sulfur* and its derivatives, while bringing us into touch with the elementary principles of phonetic reform in our own language has much broader claims on us because it so manifestly aids all users of dictionaries and indexes in English, French, German and Italian. He likewise overlooks the fact that for the same reason the *Ph* of phosphorus remains intact because Italian is thus far the only language in which the digraph has been superseded by the simple *F*, and because the change in the initial letter of a word would lead to difficulties in the matter of reference, undesirable at present.

direct need on the part of many, especially non-chemists, to have a brief summary of the reasons for the rules which have evoked the most inquiry placed in a journal reaching all classes of those interested in the progress of science.

The most important decisions may be classified under the following heads:—

1. *Elements*.—CESIUM. This shortened form for *cæsium* brings us into harmony with the French *césium*, and the Italian *cesio*, and is in accord with the prevalent reform in the use of diphthongs.

ALUMINUM. This shortened although less euphonic form meets the wishes of technical chemists, and is desirable in view of the growing industrial use of the metal.

COLUMBIUM. This replaces *niobium* as a matter of historical justice. It seems important that the one element discovered and named by an American chemist should retain the patriotic appellation first assigned it.

GLUCINUM is preferred to *beryllium* on the same ground of historical priority.

SULFUR. This is modified in accordance with the general phonetic change going on in our language, and the change is extended to all the derivatives. It is a reform which brings us into accord with the French *sulfure*, *sulfite*, etc., the German *sulfat*, *sulfid*, etc., and the Italian *zolfo* or *solfo*, *solforico*, etc. It might naturally be asked, Why not extend this reform to phosphorus? The reasons are here by no means so strong as in the case of sulfur. While the Italians use *fosforo*, the French and Germans still retain the *ph*, as *phosphore*. Again, the change would affect the initial letter—a serious matter in indexing.

2. ARSIN, STIBIN, PHOSPHIN, HYDROGEN-SULFID, etc. These shorter terms, which have long since received the stamp of authoritative usage, displace completely henceforth their cumbersome synonyms, *arsenietted hydrogen*, etc. It is hoped that the simplification may soon be carried still farther by the introduction of *sulfin*, *selenin*, and *tellurin*.

3. GRAMME. At first sight the retention of the long French form might seem inconsistent with the principles of phonetic reform actuating the changes already enumerated. It is, however, dictated by strong prudential reasons, as long as the metric system is used side by side with the old series of apothecaries' weights in medicine. As soon as the transition period is over and the latter system is effectually displaced, the simpler form will unquestionably be adopted. Such is the similarity both in sound and spelling between *gram* and *grain*, that it is evident how easily mistakes of the gravest nature could occur either in following written or verbal directions, especially in this era of telephones. It is a matter of record that several deaths have already been caused by the omission of the dot over the *i* in *grain* or by mere inadvertence.

4. Derivatives of VALENCE. In their formation the Latin prefixes are used invariably instead of the Greek, this being thoroughly in accord with the recognized principles of word-building in our language.

5. The termination -OL. This is used exclusively for alcohols, and all single names for alcohols receive the termination. This is in harmony with British usage and conduces to a most desirable uniformity and simplification. The chief difficulty in the application will be found in the use of glycerol for glycerin; but as this has been overcome in England, it certainly can be in this country.

6. The termination -IC. This is used for metals only, where there is a contrast with -ous, as in *ferric*, *mercuric*, *cupric*, etc., avoiding such forms as *strontic*, *aluminic*, *zincic*, *ammonic*, etc. The rule brings us, also, into accord with transatlantic usage and eliminates several unnecessary and far from euphonic terms.

7. The termination -IN. The changes recommended in this connection are perhaps the most far-reaching and the most subject to discussion. They involve the dropping of the final *e* from the names of all chemical elements and compounds formerly ending in -ine, and the uniform pronunciation of the final syllable with the short *i*, as *chlorin*, *amin*, *anilin*, *quinin*, *cocain*. The only exception to this rule is in the case of the group of doubly unsat-

urated hydrocarbons (butine, heptine, hexine, pentine, propine, etc.), which still retain the final *e* and the long sound of *i*. The chief objection to this rule is the fact that some years since Watts and others proposed the use of the termination -ine for basic substances and the limitation of the termination -in to certain neutral compounds, viz., the glycerids, glucosids, proteids, and bitter principles. In this latter category are found also the so-called resinoids introduced by the eclectics, and obtained by precipitating the alcoholic extract of a drug with water.

In considering the force of the objections that may be raised against the change, it must be admitted at the outset that there is an undeniable value in the consistent use of distinctive suffixes for distinct classes of compounds; provided, however, that the use of any given suffix is limited to a single class, that there is a phonetic difference as well as a visible difference between closely allied terminations, and that there is no serious violation of established usage in word-building. Illustrations of such helpful uniformity are to be found in the terminations of the various series of hydrocarbons, of the alcohols, etc. In examining how far these conditions prevail in the use of these terminations, we note that—

*a.* The use is not limited to a single class in the case of either -in or -ine. *b.* There is little or no accompanying phonetic difference, the *i* being almost invariably short. *c.* The final *e*, as a rule, when following a single consonant, should indicate the long sound for the preceding consonant (Webster's Dictionary, "Principles of Pronunciation," p. xlv.), which is not here the case. *d.* The usage would demand a very extensive and accurate knowledge of the constitution of a large number of compounds. *e.* It has been adopted by but a portion of the chemical world; few are consistent in its use; by many it has never been recognized. *f.* In the case of the resinoids, the existing possibility of danger as a result of confusion between, say, *aconitin* and *aconitine*, is but slightly helped by the presence of the final *e*, as will be easily acknowledged by anyone familiar with many specimens of handwriting, especially of physicians' handwriting, and as far as the ear is concerned remains unaffected—a most important consideration in view of the prevalent use of the telephone for ordering prescriptions.

It would seem eminently desirable for those most closely associated with the progress of pharmacy to counsel at once the abolition of this existing nomenclature as applied to the resinoids by introducing distinctive prefixes or additive terms, so as to remove entirely all possibility of confusion. An able writer in a recent article in the *American Druggist* (vol. xxi, p. 15) states: "But though they (the resinoids) are gradually going out of use, some of them are still in demand, and fatal results might ensue if both terms, that of the weaker resinoid, and that of the powerful alkaloid, were confounded." It may pertinently be inquired whether a reform, the value and utility of which is conceded by all, should be delayed by the effort to bolster up the weak fortifications about the terminology of a group of substances—not distinctive chemical compounds, but mechanical, commercial mixtures—when that terminology in its present state is confessedly a menace to human life.

The advantages accruing from the application of the new rule are, briefly stated, the following: *a.* The simplification, uniformity, and economy of time resulting from the use of a single spelling for the same sound. *b.* The unvarying use in the termination -in of the short *i*, the sound now employed in the vast majority of cases, the one approximating most nearly to the European *i*, and the one thereby most helpful to foreigners using our language, and *vice versa*. *c.* The harmonizing of the practice governing the use of this termination with the principles underlying the general rules for the pronunciation of other chemical terminations. *d.* The falling into line in this regard with the general movement towards phonetic reform in our language. *e.* The accord with the general rule in our language governing the use of the final *e* and its effect on preceding vowels.

The termination -ID. This replaces in all cases -ide (as *oxid*, *chlorid*, *sulfid*), and the *i* is invariably short. The reasons for this

rule are much the same as those enumerated in the above paragraphs. Of the three pronunciations of this termination *-ide*, *ide*, and *ide*, in varying degrees of usage amongst us, the second appeared undoubtedly to be the most preferable; *-ide* is an uncommon, almost unnatural, pronunciation of the vowel in English, although it would bring our usage into unison with that of European countries, and simplify phonetic values for the ears of foreigners; *-ide* leads frequently to confusion with *-ite*, and is the value of *i* farthest removed from European usage; *-id* approximates closely to the Continental *i*, into which it is easily lengthened, is readily recognized by the foreign ear, is not confused with the termination *-ite*, is in line with present phonetic progress, and has the backing of authority and usage. The short sound of *i* naturally dictates the dropping of the final *e*. "According to Smart and Cull, chemical terms ending in *-ide* — as bromide, chloride, etc. — should be pronounced with the *i* long; but all other orthoëpists are unanimous in making the vowel short; and the propriety of the latter mode of pronunciation is established by the fact that this whole class of words is not unfrequently spelt without the final *e*, thus *bromid*, *chlorid*" (Webster's Dictionary, "Principles of Pronunciation," p. xlv.).

In conclusion, it may be said that the chemical section of the American Association recognizes the fact that there is still room for advancement in the path of phonetic reform, and that questions may still arise with regard to divergent usage or defects in existing rules. The task of collecting and collating such questions and of presenting them at a later date to the Association for action has been assigned to Professor Jas. Lewis Howe of Louisville, who will gladly receive all information, suggestions, or propositions pertinent to the subject from those interested in the perfecting of our chemical nomenclature.

#### BOSTON SCHOOL-BOYS.

BY FRANCIS GALTON, F.R.S., LONDON, ENGLAND.

NUMEROUS results may be shown to flow from the excellently arranged data in the valuable memoir of Professor H. P. Bowditch on the Growth of Children (Twenty-Second Annual Report of the State Board of Massachusetts, Boston, 1891). Permit me to draw attention to two of them.

It is necessary to premise that the method was adopted by him of describing classes by means of eleven percentiles, but, for the present purpose, three are enough, namely, the 10th, 50th, and 90th. In other words, it is sufficient now to deal with the statures of the persons who occupy those posts in any class along whose length 100 posts have been marked at equal intervals. It follows that 10 per cent of the whole class are shorter than the 10th percentile and 90 per cent are taller. These conditions are reversed in respect to the 90th percentile; as for the 50th, it is the median value, which one half of the class falls short of and the other half exceeds. The median in most series differs little from the arithmetical mean, and may be used instead of it, as a serviceable standard of comparison.

The variability of a series may be measured by the difference between any two named percentiles. The wider these are apart the more is the scale magnified; on the other hand, the less trustworthy does the measure become. In the present series we can with propriety use the difference between the 10th and the 90th percentiles, but we cannot in all cases, owing to the paucity of data, use that between the 5th and the 95th; the former will therefore be here adopted as the measure of variability.

In order to compare on equal terms the variability in stature of growing boys at different ages we must so reduce their measures that the median shall in all cases be the same. It is customary for this purpose to take the median as 100, but there is more significance in the results when it is taken at a value that represents the average stature, or thereabouts, of male adults. Here it will be taken at 67 inches. In the following table the 10th and 90th percentiles for the several ages are those given by Bowditch, after multiplying them by 67, and then dividing the result by the median stature at that age.

Calculated from Tables by Bowditch of Heights of Boston School-boys.

Age last Birthday.	Of American Parentage.				Differences.	Of Irish Parentage.				Differences.
	Number of Cases.	Median.	Percentiles			Number of Cases.	Median.	Percentiles		
			× (67 ÷ Median).					× (67 ÷ Median).		
			10°	90°				10°	90°	
5	201	41.84	62.9	71.0	8.1	366	41.70	63.3	71.2	7.9
6	342	44.00	63.5	71.1	7.6	503	43.90	63.3	70.7	7.4
7	369	46.36	63.5	70.9	7.4	562	45.68	63.5	70.9	7.4
8	407	48.34	63.2	70.9	7.7	588	47.80	63.6	70.6	7.0
9	381	50.07	63.5	71.2	7.7	556	49.61	63.7	70.9	7.2
10	360	52.24	62.7	70.5	7.8	571	51.62	63.6	71.1	7.5
11	350	54.14	62.5	70.5	8.0	548	53.17	63.4	70.6	7.2
12	373	55.68	63.4	72.2	8.8	497	54.89	63.2	70.9	7.7
13	391	58.14	62.3	71.8	9.5	463	56.58	63.7	71.9	8.2
14	386	60.77	62.6	72.8	10.2	334	58.81	63.3	71.5	8.2
15	342	63.17	62.3	71.4	9.1	155	60.98	63.0	71.9	8.9
16	232	66.03	62.5	69.9	7.4	61	64.42	(61.8)	(70.4)	(8.6)
17	128	69.39	63.2	71.0	7.8	26	Too few.			(?)

On examining the columns of differences, we find a remarkable increase in the differences between the 10th and 90th percentiles during the interval between the ages of 11½ and 15½ years; that is, of boys who at their last birthday were 11 or 15 years old. The period in question is that during some portion of which the growth is apt to be temporarily accelerated, but the precise epoch of acceleration differs; some boys being more precocious than others. Consequently the variability among boys of the same age, between the ages of 11½ and 16½ years, is greater than at other times. The point to which I wish now to direct attention, is the much greater variability during this period of the children of Americans than of those of Irish, for which it seems difficult to account. It can hardly be owing to variations of nurture, because its influences would probably be greatest on those classes who were least assured in their habits of life; now it is difficult to suppose that the Irish in Boston are, as a class, better established and more well-off than the Americans. As regards the effects of race, it is true that the Americans are more mixed in origin than the Irish, but we should have expected purity of race to manifest itself by a reduced variability at all ages, and not only at the particular period we are considering. However, it seems to be otherwise, and that the great variability of American children at the time in question may really be due to their mixed ancestry. In confirmation of this variability being a racial effect, we note how much earlier the epoch of its increase sets in among the children of Americans than among those of Irish, the difference amounting to at least one year. Anyhow, these statistics suggest the possible existence of an hitherto unobserved physiological difference between the children of the Americans and of the Irish, which might repay investigation.

A considerable agreement will be found in the figures contained in each of the four columns of percentiles in the table; their variations ranging through 1.2, 1.9, 0.7, and 1.3 inches, respectively. In other words, they range between limits that are hardly more than one inch on the average apart, while of course the range in other percentiles that are nearest the median is progressively smaller, till at the median itself the range is *nil*. There is, therefore, a fair approximation towards constancy in the ratio between any given percentile and the corresponding median that holds good for all these ages. It follows that if we are given all the eleven percentiles of stature that are found in Bowditch's memoir, together with the median heights for the several successive ages, we should have sufficient data to reproduce, in a roughly approximate way, the entire table of distribution of growth. The variability and the median are not such independent



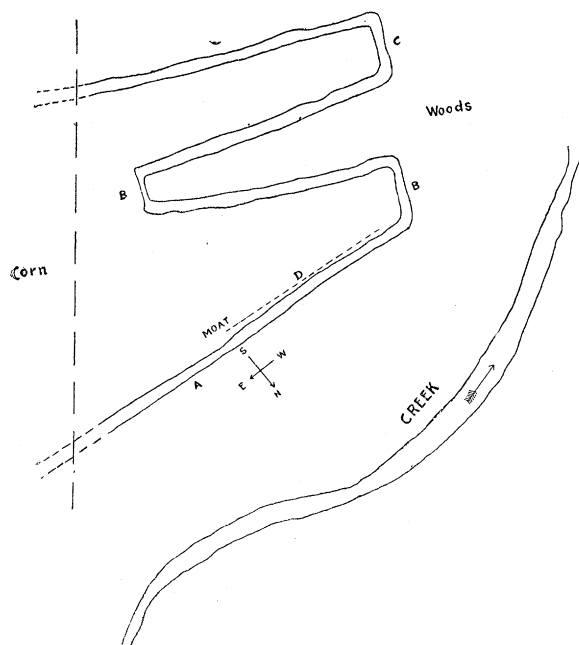
values as they are commonly considered to be. As, for example, the amount of the changes in the length of a chain under the influences of changing temperatures is related to the length of the chain, so we should expect the variability in the growth of large organisms to be on a larger scale than in small ones. There are more cells affected simultaneously by the same enviroing conditions. The rationale of a connection between the variability and the median may in some cases admit of being clearly made out, and in all cases it deserves more thought than it has hitherto received.

### AN ARCHÆOLOGICAL DELUSION.

BY THOMAS GORDON KING.

THE daily papers have lately published accounts of a new "serpent" effigy. It existed in southern Ohio, in Warren County, and, according to two doctors of the neighborhood, measured some nineteen hundred feet in length. It was said to surpass the famous Adams County serpent.

Professor Putnam's assistant, Mr. H. I. Smith, spent some time surveying the structure, this summer. He trenched the embank-



ment in several places and searched the neighboring fields for traces of a village site. The accompanying rough outline gives an idea of the "serpent" so far as it can be traced. In the corn-field it will be seen that the embankment cannot be distinguished; in the woods it is plain. The part in the woods, which at present is some two feet in height, does not appear to be serpentine in character. It is almost unnecessary to add that if the remaining part of the structure does not represent a serpent, the obliterated portion never did. There is not the slightest grounds for the assumption that this figure in any way resembles the Adams County effigy. The latter is laid out in graceful curves, which suggest the character of the effigy. The embankments of the Warren County structure resemble those of Fort Ancient. The long straight line A, and the sharp, squared bends B and C are the exact counterpart (although much smaller) of certain parts of south Fort Ancient.

A live snake could not take the form of this "new serpent" without breaking his back in three places. (I write under the impression that aborigines imitate living and not dead animals). There is a slight moat at the base of the embankment, which, although nearly filled, can still be traced. To one who has seen all the shell, bone, stone, and clay representatives of serpents and serpent-symbols displayed in the museums of this country, the "new serpent" does not appear serpentine. I cannot see how the angular

corners B and C and the moat D, and the embankment A, mark other than parts of a peculiar defensive earthwork.

The primitive Americans in drawing, moulding, building, or sculpturing snakes evinced a certain similarity of idea in design, and employed a common mode of execution. Yet this "new serpent" has nothing in common with other serpents! (Read Holmes on "Art in Shell.") As this new serpent is such a poor representative that Professor Putnam and other competent judges dare not place themselves on record in naming it, I have no hesitancy in calling it a rude fortification. The native Americans were sufficiently competent to execute a figure with such distinctness and closeness of resemblance as would allow of no dispute. Those who are interested in following the discussion further will please compare the diagram submitted with Squier and Davis's plan of the Adams County effigy. There are many similar combination works in the Ohio Valley, and it is probable that the thorough exploration of several might furnish evidence as to the purpose for which they were erected.

### LETTERS TO THE EDITOR.

#### Man and the Glacial Period.

I ACKNOWLEDGE with pleasure the courtesy with which Dr. Brinton, in his review of "Man and the Glacial Period," has dealt with the question of the genuineness of the reported discoveries of implements in the glacial gravels of the United States. This, of course, was the first question to be settled, Were implements of human manufacture really found in undisturbed strata of gravel which was deposited during the glacial period? If this question is settled in the affirmative, then all glacial geology has direct bearing upon the question of archæology. If it is decided in the negative, glacial geology remains the same, but it ceases to have interest in connection with archæology. I am glad to have the issue so clearly made by Dr. Brinton, and thereby to have occasion to present more specifically my reasons for belief in the genuineness of these discoveries.

The evidence naturally begins with that at Trenton, N. J., where Dr. C. C. Abbott has been so long at work. Dr. Abbott, it is true, is not a professional geologist, but his familiarity with the gravel at Trenton where he resides, the exceptional opportunities afforded to him for investigation, and the frequent visits of geologists have made him an expert whose opinion is of the highest value upon the question of the undisturbed character of the gravel deposit. The gravel banks which he has examined so long and so carefully have been exposed in two ways: 1st, by the undermining of floods on the river side, but principally by the excavations which have been made by the railroad and by private parties in search of gravel. For years the railroads have been at work digging away the side of the banks until they had removed a great many acres of the gravel to a depth of twenty or twenty-five feet. Anyone can see that in such conditions there has been no chance for "creep" or landslides to have disturbed the stratification; for the whole area was full of gravel, and there was no chance of disturbance by natural causes. Now Dr. Abbott's testimony is that up to the year 1888 sixty of the four hundred palæolithic implements which he had found at Trenton had been found at recorded depths in the gravel. Coming down to specifications, he describes in his reports the discovery of one (see "Primitive Industry," 492) found while watching the progress of an extensive excavation in Centre Street, which was nearly seven feet below the surface, surrounded by a mass of large cobble stones and boulders, one of the latter overlying it. Another was found at the bluff at Trenton, in a narrow gorge where the material forming the sides of the chasm had not been displaced, under a large boulder nine feet below the surface (ib. 496). Another was found in a perpendicular exposure of the bluff immediately after the detachment of a large mass of material, and in a surface that had but the day before been exposed, and had not yet begun to crumble. The specimen was twenty-one feet from the surface of the ground.

In all these and numerous other cases Dr. Abbott's attention was specially directed to the question of the undisturbed char-

acter of the gravel, having been cautioned upon this point in the early part of his investigations.

Nor is he the only one who has found implements which were clearly in those undisturbed gravel deposits. Professor Shaler (Report of the Peabody Museum at Cambridge, Vol. II., p. 45) found two of the implements twelve feet below the top of the bank, where he says that it was difficult for him to believe that they could have travelled down from the superficial soil, and he expresses it as his opinion, after having gone over the ground with Dr. Abbott, that the implements which Dr. Abbott had found occurred under conditions that make it "quite unquestionable that they were deposited at a depth of many feet below the soil, and are really mingled with the drift matter that forms the section before described." This is the description which I have quoted in my volume (p. 242). Professor Putnam, also, personally found implements in position which he decided to be certainly undisturbed gravel (see 14th Annual Report of Peabody Museum, p. 23, and Proc. Boston Society of Natural History for Jan. 19, 1880).

The question of the occurrence of these implements in undisturbed gravel was so thoroughly discussed by the scientific men in Boston who visited the region about 1880 that I had supposed there was no longer any reasonable doubt concerning the facts, and I feel sure that anyone who goes through the records of the Peabody Museum and the Boston Society of Natural History about that time will be convinced. At the same time I would say that I have been unable myself to find any implements in place, though I have frequently examined the bank. But I have not felt at liberty on that account to doubt the abundant testimony of others who have. If we are limited to believing only what we ourselves have seen, our knowledge will be unduly circumscribed; and though I might be more certain of the facts if I had seen them myself, I do not see how I could increase the confidence, in the facts, of other people who could disregard the testimony already in hand.

Passing now from the discoveries at Trenton, N. J., to those in gravels of corresponding age in Ohio, we do not come to the subject with the same amount of incredulity with which we first encountered the evidence at Trenton. Dr. Metz has been for years co-operating with Professor Putnam in various investigations, and the discovery of a flint implement by him in excavating for a cistern in his own yard was such that no reasonable question can be raised as to its having been undisturbed since the deposit was made, and there can be no reasonable question that the deposit was made during the continuance of glacial conditions in the State. I have described the conditions in a report to the Archaeological Society of Ohio for December, 1887.

The discovery of a palæolithic implement at New Comerstown, Ohio, by Mr. W. C. Mills, is an equally well-attested case. Mr. Mills, like Dr. Abbott, resided in close proximity to an extensive glacial terrace to which the railroad was resorting for ballast. Many acres of the gravel have been removed. During the progress of these excavations Mr. Mills repeatedly visited the pit, and after a fresh excavation discovered this implement in a perpendicular face of the bank fifteen feet below the surface. The facts were recorded in his diary and the implement placed in the general collection of Indian relics which he was making. Mr. Mills was at that time engaged in business in the place, but he had been a pupil of Professor Orton in geology, and was well qualified to judge of the undisturbed character of the gravel in which this implement was found. As anyone can see by consulting the photographic illustrations on pp. 252 and 253 of my volume, the implement itself is an exact duplicate, so far as form is concerned, of one which I have in my own collection, from Amiens, France, and which came to me, through Professor Asa Gray, directly from the collection of Dr. Evans in London. The New Comerstown implement was submitted to Professor Haynes of Boston and to others at a meeting of the Boston Society of Natural History, and by them pronounced to have all the essential characteristics of palæolithic age. The full report upon this is found in Tract No. 75 of the Western Reserve Historical Society, Cleveland.

As to Miss Babbitt's discoveries at Little Falls, Minn., I have

nothing further to say than that up to the present year no serious question had been raised, concerning the glacial age of the gravel in which her implements were found. But as questions have now been raised in view of recent examinations, I will not attempt to discuss the matter until the facts are more fully published. But the removal of this case from the category would not disturb confidence in the evidence connecting man with the glacial period in New Jersey and Ohio.

The statement of Dr. Brinton that a well-known government geologist had recognized the Nampa image "as a clay toy manufactured by the neighboring Pocatello Indians" is news to me, and it is due to the public that this official's knowledge of the subject should be more specifically detailed. The facts as I have brought them out by prolonged and minute inquiry do not warrant any such flippant treatment of the evidence. Professor Putnam, to whose inspection the image was subjected when it first came into my hands, at once pronounced it an antiquity of some sort, unlike anything which he knew to be in existence among the aboriginal tribes. I need not say that Professor Putnam's opinion upon a question of that sort is of the very highest value. There were upon the image patches of the anhydrous oxide of iron, which to him and other experts were indubitable evidence that it had lain for a long time in the earth. Subsequently I ascertained, while on the ground at Nampa, that the shade of color in this iron oxide upon it corresponded exactly to that which had formed upon the clay concretions which came up in large quantities from the same stratum in which the image was alleged to have been found. I have also, I think, made it evident that the burying of human relics even to the great depth of 320 feet in the Snake River Valley may not be much more surprising than the burial of the remains of man in Pompeii and Herculaneum, and that the date of this burial may not have been very many thousand years ago. The direct evidence to the fact that this little image, an inch and a half long, came up from the depth reported is about as convincing as we can have for any fact which depends for credence upon human testimony. There has been nothing with regard to the appearance of the parties suggesting fraud. Mr. Cumming, the superintendent of that division of the Union Pacific Railroad, whose attention to the facts was called the day after the discovery, is a Harvard College graduate, of extended legal education and wide practical experience, who knew all the parties and was familiar with the circumstances, and investigated them upon the ground. Charles Francis Adams emphatically affirms that Mr. Cumming's evidence in this matter is entitled to as much consideration as the evidence of any scientific man would be. Anyone who wishes to get my detailed report of the evidence will find it in the Proceedings of the Boston Society of Natural History for Jan. 1, 1890, and Feb. 18, 1891.

The discoveries of human implements under Table Mountain in California are in close analogy with this discovery at Nampa, in the Snake River Valley, and the same remarks have been made respecting them that Dr. Brinton reports concerning the Nampa image, namely, that they are modern implements at present in use among the local tribes of Indians. But no such offhand opinion as this can break the force of the evidence which has accumulated in support of their having been found in deposits which have been undisturbed since the great lava outflows which constitute what is called the Sonora Table Mountain. The evidence concerning the Calaveras skull has been exhaustively discussed by Professor Whitney of Harvard College, who pronounces the facts to be beyond all reasonable doubt. At the meeting of the Geological Society in Washington in January, 1891, three independent discoveries of human implements in conditions similar to those assigned to the Calaveras skull were presented. I had myself obtained information at Sonora of the discovery of a stone-mortar in the tunnel of the Empire mine of which the evidence was satisfactory beyond reasonable doubt. The discovery was made by the assistant surveyor of the county in the tunnel of a mine under Table Mountain, which was owned by his father and where work is still prosecuted. The mortar had been given away to another person, but it has since come into my hands and is preserved in the Museum of the Western Reserve Historical Society of Cleveland.

At the same meeting Mr. George H. Becker of the U. S. Geol. Survey presented a similar mortar found under Table Mountain some years before by Mr. Neale, a mining engineer. Mr. Neale signed an affidavit detailing the particulars, and his remembrance of the situation was so minute that there could be no question of the undisturbed character of the deposits. Mr. Becker well remarks that Mr. Neale's judgment as mining-engineer concerning the undisturbed character of the deposit is the highest evidence that can possibly be obtained, for that is a point to which the miner's attention is constantly directed, on account of the danger attending the opening of any old excavation.

The third new evidence offered was that of Mr. Clarence King, who had just presented to the Smithsonian Institution a fragment of a pestle which he had taken with his own hands, in the vicinity of the two previous places mentioned, from the undisturbed gravel beds underlying Table Mountain. I need not say that Mr. Becker and Mr. King are two geologists of the very highest standing in the country, and that they both have unusual familiarity with the phenomena of that region, and they both, together with Professor Marsh, Professor Putnam, and W. H. Dall, express their unqualified belief in reference to the Calaveras skull that it was found in place in the gravel beneath this same stream of lava.

But I have already made my communication too long. I trust, however, upon your forbearance in publishing it, since the facts are too numerous to be compressed into less space of description, and a volume would be required to give all the evidence in detail. In my book upon "Man and the Glacial Period" I was called upon to discuss a very broad subject in a very small volume, and so could not enter into details. I endeavored, however, to limit myself to facts of which there was abundant proof, if they should ever be called in question. And I would repeat that I am glad of the revival of interest in the subject which will be created by the expression of such doubts as still remain in Dr. Brinton's mind. I have no question but full discussion will dispel the uncertainty that may exist.

G. FREDERICK WRIGHT.

Oberlin, Ohio, Nov. 1, 1892.

#### The Rattlesnake of the Bottom-Lands of Mississippi.

ON August 8, 1891, I received, in a box whose base was about two feet square and whose height was about one foot, a rattlesnake which had been sent by express from Greenville, Miss. The snake was of a stout build, fierce looking and ready to rattle and strike. Through this wooden box had been bored a number of auger holes to supply the serpent with air; these had so weakened the box as to cause a split that afforded some chance of escape.

I thought from its appearance the snake was about four feet long; but after death actual measurement showed four feet five inches in length and eight inches in circumference at the largest part of the body. I had made a longer box with glass top and with a sliding door; through this without much difficulty the snake was transferred from the old box.

The color was an alternation of black spots and light brown ground. The black spots were larger than those of the Mountain Rattler, while the brown was not so bright. This snake had only three rattles when I received it; a careful examination showed that some of the rings had been broken off. I afterwards learned that eight of them were broken off in the express car between Greenville and Winona on the Georgia Pacific road. If this information be correct, the snake had at that time eleven rattles.

When I was endeavoring to make the snake go out of the old box into the new, the glass of the latter was at the side. The snake showed evidence of great irritation and anger; it rattled almost incessantly. Some children were a few feet in front of the glass; at the instant of passing into the new box it struck at the children with all its force, striking against the glass and spattering against it some white, thick, frothy liquid. The snake evidently did not understand glass, not having lived before in a box or house provided with that article. Twice afterwards, when it had been angered, it struck at persons standing a little way in

front of the glass; after this it refused to strike, seeming to understand that the glass presented a barrier too great for its strength. The head of the snake trembled from the effects of the blow against the glass. A match struck and lighted in front of the glass seemed to irritate and anger it more than anything else.

I took the precaution to have in the cage a wooden saucer. In this I blew at various times water and sweet milk. I put into the box living frogs, rats, mice, young flying-squirrels, chickens, etc. I also offered to it frequently fresh butcher's meat; but it refused all food; it evidently had no appreciation whatever of any attention or effort to be kind. A young chicken was bitten by it and was dead in twenty seconds; it fell instantly on being struck by the serpent's tooth. Two toads at different times died in the box after remaining therein about six days each. Several times I poured clean water into the box and on the snake; this made it move restlessly; it pushed its nose tremulously against the glass, and, slid it along the glass as if trying to break the glass or find an opening for escape. It seemed to reject water as indignantly as it did food. When lying in the box it seemed to be the perfect expression of sullen disdain.

During October it shed its skin partially. The work of shedding began in the night but it was never perfectly done, parts of the old skin adhering to the sides of the body. During its confinement a new rattle was formed between the former rattles and the body proper, showing thereby that the terminal rattle of the rattlesnake is the oldest.

As time went on the snake became poorer, but the skin was so thick and scaly that the ribs were not visible, and when it was irritated the body was distended to its full former size, either by the drawing and rigidity of the muscles or by inflation. Without food, without water, confined in a box and subject to some considerable variations of temperature, it lived from August 8, 1891, to April 15, 1892, eight months and seven days. During the winter the room in which it was kept often grew cold, but I never allowed it to be cold enough for water to freeze. When in the cold, it coiled closely and seemed torpid; but, on my moving the box into a warm room, it would very slowly uncoil and stretch itself in its box almost straight.

I have heard many extravagant stories about the length of time a rattlesnake could live without food, but I was not prepared to believe that it could live as long as eight months and seven days, until the fact was demonstrated as I have narrated above. Mr. W. W. Stone, the Auditor of Mississippi, who sent this snake to me, informed me it was without food at least a week before I received it. In feats of fasting this animal excels Dr. Tanner and all other human aspirants for that kind of distinction so far as to make their boasts futile.

R. W. JONES.

University of Mississippi.

#### Preliminary Note on Sleep.

THAT there is a relative anæmia of the brain during sleep is well established, but the hypotheses advanced to account for this or any other of the sleep phenomena are unsatisfactory. In "Comparative Physiology and Psychology," 1884, I treated the subject briefly, and since then have been gradually accumulating and arranging data for a theory which I have finally adopted, and which appears to me to be fairly complete as enabling the major phenomena to be accounted for.

Briefly stated, where there is physiological waste there is, normally, repair, and the activities of the brain demonstrably are kept up by renewed nutrition derived from a blood supply adjusted to the ordinary needs. When there is cerebral anæmia, as in chlorosis, then there is increased desire to sleep, the brain does not receive the necessary quantity to compensate waste, and it rests, just as any commercial activity will cease with withdrawal of means to continue it. Those who are familiar with my nutrient reflex theory, mentioned in the book referred to (Professor C. K. Mills of the Pennsylvania University, and Professor C. L. Herrick of the Denison, Ohio, University, have written approvingly thereon), will understand that with cessation of sensory stimulation there will be less blood attracted to the brain and other nerve-centres, the heart-beats lessen in vigor and num-

ber, and, with the pulse-rate full, there is ordinarily less blood in the brain.

Now, it is evident that the anæmia of sleep is not caused by constricted blood-vessels, else there would be the facial pallor seen during an attack of epilepsy, or paroxysm of anger or fright; and with this quieting of the brain-processes by stimuli withdrawal, such as is afforded by darkness, silence, and absence of irritation generally, a further lessening of molecular interchange in the brain occurs; and, I claim that it is the molecular activity in the brain that attracts the blood there chemically and mechanically, and the sympathetic, or vaso-motor system has evolved to facilitate this regulation of demand and supply. Then, granting this, there will be, during sleep, a passive condition of the blood-vessels, and the blood supply will fall to a minimum.

An extension of these considerations will enable all that pertains to sleep to be accounted for, such as æstivation, hibernation, insomnia, dreams, and all derangements of sleep. I hope soon to be able to treat this subject more fully.

S. V. CLEVINGER.

Chicago, Oct. 15.

### Solid Glycerine.

IN response to the inquiry of Mr. C. C. Smith regarding the solidification of glycerine, I would say: A mixture of glycerine with water can be frozen at a sufficiently low temperature, and this temperature must be the lower proportionately as the percentage of glycerine is high. Thus, a ten per cent glycerine solution solidifies at  $-1^{\circ}\text{C.}$ , a twenty per cent solution at  $-2.5^{\circ}\text{C.}$ , a forty per cent solution at  $-17.5^{\circ}\text{C.}$

Concentrated glycerine will not crystallize when cooled quickly, but at  $-40^{\circ}\text{C.}$  will solidify to a gum-like mass. If a concentrated solution be allowed to stand for some time at  $0^{\circ}\text{C.}$  crystals may form, but not always. The melting-point of these crystals, which are extremely hygroscopic, has been variously determined; and, indeed, their form of crystallization is much in dispute.

Two cases are reported of glycerine having become solidified and crystallized during transport in the cold of winter. The first case occurred in January, 1867, the crystals formed being described as small octohedral, melting at  $7.2^{\circ}\text{C.}$  In the second case, 1876, the crystals are described as belonging to the monoclinic system, and melting at  $15^{\circ}\text{C.}$

According to Werner, commercial glycerine may be crystallized by bubbling chlorine-gas through it. A method discovered by Kraut in 1870, but to the best of my knowledge not yet made public, is used on a commercial scale in the works at Liesing, near Vienna. The concentrated glycerine is cooled to  $0^{\circ}\text{C.}$ , and maintained at that temperature for some time, when crystals of glycerine previously produced are introduced. This causes a crystallization of the entire mass, leaving, however, much of the impurity in the mother liquor. The mass is then placed in a centrifugal, and the crystals freed. These are described as monoclinic, melting at  $20^{\circ}\text{C.}$  to glycerine of  $30.5^{\circ}\text{B.}$  According to Von Lang, the crystals are orthorhombic.

CHARLES PLATT, A.C.

The Vandenberg Laboratory, Buffalo, Nov. 8.

### BOOK-REVIEWS.

*Crania Ethnica Americana. Sammlung Auserlesener Amerikanischer Schädeltypen.* Herausgegeben von RUDOLF VIRCHOW. Mit 26 Tafeln und 29 Text-Illustrationen. Large 4to. Berlin, A. Asher & Co., 1892. 36 marks.

SINCE the publication of Dr. Morton's "Crania Americana," now more than half a century ago, there has been no contribution to American craniology at all comparable to this work by the acknowledged master of that science in Germany. Future investigators will undoubtedly follow the lines and be guided by the principles here laid down or suggested. Let us briefly see what these are.

### CALENDAR OF SOCIETIES.

#### Biological Society, Washington.

Nov. 5.—C. Hart Merriam, The Fauna and Flora of Roan Mountain, N.C.; C. V. Riley, Pea and Bean Weavils; Vernon Bailey, The Influence of the Cross Timbers on the Fauna of Texas; Theobald Smith, On Certain Minute (Parasitic?) Bodies Within the Red Blood Corpuscles.

#### New Mexico Society for the Advancement of Science, Las Cruces, New Mexico.

Nov. 3.—J. P. Owen, Notes on the Mound Builders; C. H. Tyler Townsend, A Partial Comparison of the Insect Fauna of the Grand Cañon with that of the San Francisco Mountain, in Arizona; Arthur Goss, The Exhaustion and Renewal of Soils; C. T. Hagerty, Mathematical Computation of the Comparative Strength of Insects and the Higher Animals.

### Publications Received at Editor's Office.

BUBIER, E. T., 2nd. Questions and Answers About Electricity. New York, D. Van Nostrand Co. 16<sup>p</sup>. 100 p. Ill. 50 cts.  
CHURCH, ALFRED J. Stories from the Greek Comedians. New York, Macmillan & Co. 12<sup>p</sup>. 350 p. Ill. \$1.  
CROCKER, F. B. AND WHEELER, S. S. The Practical Management of Dynamo and Motors. New York, D. Van Nostrand Co. 12<sup>p</sup>. 100 p. Ill. \$1.  
FERREE, BARR. Comparative Architecture. New York, The Author. 8<sup>p</sup>. Paper. 15 p.  
GALTON, FRANCIS. Hereditary Genius. 2d ed. New York, Macmillan & Co. 8<sup>p</sup>. 411 p. \$2.50.  
HORN, ARTHUR H. Metal Coloring and Bronzing. New York, Macmillan & Co. 16<sup>p</sup>. 352 p. \$1.  
HUDSON, W. H. The Naturalist in La Plata. London, Chapman & Hall. 8<sup>p</sup>. 392 p. \$3.  
SLOANE, T. O'CONNOR. The Standard Electrical Dictionary. New York, Norman W. Henley & Co. 12<sup>p</sup>. 624 p. \$3.  
SPEAR, MARY A. Leaves and Flowers. Boston, D. C. Heath & Co. 12<sup>p</sup>. 103 p. 30 cts.  
UNIVERSITY OF PENNSYLVANIA. Contributions from the Botanical Laboratory. Phila., The University. 8<sup>p</sup>. Paper. 72 p. Ill.

### Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For Sale or Exchange.—The undersigned has a lot of first-class duplicate bird's skins and sets of eggs, both rare and common, for sale or acceptable exchange. Also about two hundred second class skins and five hundred eggs, suitable for study specimens, at very low figures. The latter, for starting a collection, are as good as the best, embracing all classes and nearly all families. Also about forty species of fossils, principally Devonian. MORRIS GIBBS, M.D., Kalamazoo, Mich.

For Exchange.—"The Birds of Kansas."—Goss, for Gray's Anatomy, or Medical Dictionary. Must be in good condition. Address, J. H. SIMPSON, Buchtel College, Akron, Ohio.

For Sale or Exchange.—The subscriber would like to receive cash offers, or an offer in exchange for the earlier volumes of Poggendorf's Annalen and the later volumes of Silliman's Journal, upon the following list: Chenn—Manuel de Conchyliologie. 2 vols. Nearly 5,000 figures, some hand-colored Paper. Paris, 1859. Edwards.—Butterflies of N. A. 2 vols. Plates hand-colored. Vol. I., half calf. Vol. II. in parts. Leyman, Agassiz, Hagen.—Ills. Cat. Mus. Comp. Zool. at Harvard. No. I. Ophiuridae. No. II. Acalephae. No. III., Astacidae. All bound in one volume. American Naturalist. Vols. I.—VII. Cloth. Silliman's Am. Jour. of Science and Arts. Third Series. Vols. I.—X. Cloth. Binney.—Terrestrial Mollusks of N. A. Colored plates. 4 vols. Stretch.—Zygaenidae and Bombycidae of N. A. Colored plates. Also a considerable library of monographs, reports, and scientific books, and a large number of duplicates of fossils, minerals and shells. E. A. STRONG, Ypsilanti, Mich., Sept., 1892.

### Reading Matter Notices.

Ripans Tabules: for torpid liver.

Ripans Tabules banish pain.

### Wants.

A POSITION is desired in the South, preferably the Gulf States, where I can teach the sciences. Can also instruct in other branches. Salary only nominal, as I am simply desirous of employment while spending the winter in the South. A private family preferred, but will accept regular school work if not too confining. MORRIS GIBBS, M.D. Kalamazoo, Mich.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

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CHEMIST AND ENGINEER, graduate German Polytechnic, Organic and Analytical, desires a position in laboratory or chemical works. Address 213½ E. 7th Street, New York, care Levy.

## The American Geologist for 1892.

Edited by PROF. S. CALVIN, University of Iowa; DR. E. W. CLAYPOLE, Buchtel College; JOHN EYERMAN, Lafayette College; DR. PERSIFOR FRAZER, Penn. Hort. Soc.; PROF. F. W. CRAGIN, Colorado College; PROF. ROBT. T. HILL, U. S. Irrigation Survey; DR. ANDREW C. LAWSON, University of California; R. D. SALISBURY, University of Wisconsin; JOSEPH B. TYRRELL, Geol. Sur. of Canada; E. O. ULRICH, Minnesota Geological Survey; PROF. I. C. WHITE, University of West Virginia; PROF. N. H. WINCHELL, University of Minnesota. Now in its IXth volume. \$3.50 per year. Sample copies, 20 cents. Address

THE GEOLOGICAL PUBLISHING CO., Minneapolis, Minn.

Dr. Virchow aims in the first place to establish a series of cranial ethnic types as the foundation of ethnic classification. Here, very much depends on the sense in which a "type" is understood. For him, it is the sum of those traits which belong to the crania of a given ethnic division, excluding, on the one hand, traits which are individual, and, on the other, those which are generic. Theoretically, it is the expression of the ethnic law of hereditary development, which, independently of outward circumstances, controls bodily growth. It must be defined by a series of exclusions and averages.

Each of the "types" which he figures is represented with the utmost fidelity in five different positions, showing the following norms: norma frontalis, occipitalis, temporalis, verticalis, and basilaris. All are represented from the points of view of the "German horizontal," which is a line drawn from the superior point of the external auditory foramen to the lower margin of the ocular cavity. This method of iconography is in itself worth a particular study, and no exception can be taken to its accuracy and its superiority to those heretofore in use.

Close attention is given to artificial deformations of the skull, which were numerous and widespread among the American aborigines. Eight leading varieties are classified and their effects analyzed. It is shown that by laws of compensatory growth such deformity does not entail diminished cubical capacity. The lowest capacity, 1,100 cubic centimeters, was in a normal skull from Chile; the highest, 1,880 cubic centimeters, was in a Labrador Eskimo.

The general conclusions reached by this masterful study will interest every one. In the first place, Dr. Virchow denies that there is any one characteristic aboriginal American type of skull, or, so far as one can see, that there ever has been one. The salient traits, none of which is peculiar to the race, are the os Incae, which is an arrest of development; the absence of the temporal process, the presence of which is a pithecoïd trait; exostosis of

the meatus auditorius, which must be regarded as pathological; and certain changes in the alveolar and malar bones, largely due to function. Finally, the conclusion is reached that the lowest known forms of the human skull have no counterparts in any yet discovered in America, and therefore we must at present draw the inference that such types did not exist there, and that the oldest history of the human species will not be enlightened by any discoveries in the New World. Man came to America as an immigrant, physically highly developed, and doubtless in a condition of culture corresponding thereto.

This brief outline gives but a faint idea of the riches offered in Dr. Virchow's introduction, which is written, moreover, in that lucid and vigorous style of which he is such a master, and which is in such happy contrast to most German scientific composition. There are, however, a few points where the work is open to question. The reference on the first page to the inferior value of linguistic grouping carries with it its own condemnation; for where there is mixture of languages there is invariably mixture of blood, and hence of cranial types as well. Bones cannot guide us better than roots in such interminglings. In several of his "types" the history is sadly incomplete. Thus, Plate xv. shows a "Mexican" skull; but to say "Mexican" is every whit as vague as to say "European;" and what guarantee have we that its peculiarities are not individual instead of ethnic? This observation applies to several other of the crania figured. According to his own definition of type, such specimens can have but very doubtful value. In the text to Table xx. it is assumed that a prominent frontal protuberance is a proof that the skull belonged to a male. Do other anatomists concede this? But criticism is disarmed by the candid statement of the author that the material at hand was far from sufficient to reach the point he desired, and that his work must be regarded rather as a preliminary contribution to the study of this wide and important field. This it is, in the best sense of the word.

D. G. BRINTON.

## Dyspepsia

Dr. T. H. Andrews, Jefferson Medical College, Philadelphia, says of  
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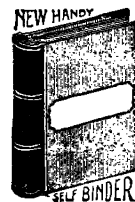
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